

Interactive comment on “Rapid attribution of the August 2016 flood-inducing extreme precipitation in south Louisiana to climate change” by K. van der Wiel et al.

Anonymous Referee #2

Received and published: 20 November 2016

The study aims at attributing the rainfall leading to widespread flooding in Louisiana in August 2016 to anthropogenic climate change. At least according to the title of the study. The manuscript provides a thorough analysis of the extreme rainfall event and includes an assessment of the role of anthropogenic climate change as well as El Nino. The attribution part of the presented manuscript could be stronger and the group has presented better studies in terms of robustly attributing the role of anthropogenic climate change as this study is primarily based of one model and focused more on a general climatological context than the anthropogenic signal per se. Analyses of the British rainstorm and the French rainfall extremes submitted to the same journal by a similar set of authors better harness the power of multiple methodologies and multi

[Printer-friendly version](#)

[Discussion paper](#)



models. I realise I disagree with some vocal colleagues on this aspect thus it's worth highlighting. Adding for example an assessment based on the very large ensembles from the Oxford group which allow for a clear separation of the anthropogenic climate signal as in previous studies could have made the attribution statement stronger (even if the statement would have been that the model doesn't capture the events at all but judging from their website a regional model in the right part of the world seems to exist).

The presented studies is however a very strong assessment of the rainfall leading to the Louisiana floods in terms of the nature of the extreme event, the atmospheric processes and the rareness of the event in today's climate making it a valuable contribution to the literature on such extremes. In particular the clearly presented model evaluation and a presentation of the assumptions that went into the conclusions could serve as a model how these kinds of analyses could and maybe should be done in the future. I would thus recommend only relatively minor revisions to make the presentation of results clearer but would advise on changing the title to make it clear that the attribution to man-made climate change is not the main focus but rather an autopsy of the event in a climate context.

Furthermore, currently the discussion section is rather lengthy and while of very high quality in the first part when describing the assumptions made in the meteorological analysis and their discussion the second part (7.2) relating to the impacts of such an event does not provide any original analysis but rather common place arguments that apply in any extreme event context. This stark decrease in the quality of the discussion does not serve the overall paper well and leaves the reader unnecessarily disappointed when reading the end of a very good paper. I would thus recommend drastically shortening section 7.2 and simply stating that in the future an analysis of the specific vulnerabilities and exposure in the impacted region would enhance the direct applicability of the meteorological event autopsy. The latter part of the section is better placed in the methodology section but has already been mentioned there.

Specific comments

[Printer-friendly version](#)

[Discussion paper](#)



p.2, l.35 – strange grammar, rephrase sentence

p.2, l.52 – strange grammar, rephrase

p.3, l.72- explain “flood stage”

p.4, caption figure 2 – what is the pink area?

p.4, ll.86-92 – while not irrelevant, the introduction is already very long, to actually have someone read this interesting paper it would be a good idea to shorten the introduction, this paragraph provides a good opportunity, but not the only one

p.5, l.124 – add reference to what to examples or more explanation of what is meant by thermodynamic and dynamic responses and ‘weather type’. In particular the latter is quite ambiguous

p.6, ll.143/144 – to avoid even more confusion than already exists in the community the term “event attribution” with or without added “extreme” or “probabilistic” would be better

p.7, ll.181-184 – what are the criteria to through out the other two considered models?

p.7., l.198 & above – flux adjustment is not done in many models anymore for good reasons, very briefly discuss what the implications are for your study

p.9, l.241 – mention that in 7.1 you’ll find the assumption not unjustified

p.10, l.266 – add “a” between has & realistic

p.10, l.270 – do you mean the Golf region?

p.11, l.295 – add reference for the moving block technique

p.13,l.339 – why do you use 3-day averages instead of sums?

p.13,ll.352-355 – is there a reason for expressing the change in probability by a factor and the intensity in percentages? If not use the same measure for both.

Printer-friendly version

Discussion paper



p.14, l.361 – In which of the 2 sentences does figure 1 d,f belong?

p.24,l.542 – up to 2100

p.25, ll.552-553 – this is a much better expression of your bias correction method than before

p.26,l.580 – is this a justified assumption?

p.28, table 3 – make clear in the table which experiment provides the proper attribution analysis

p.29, figure 15 – nice figure, but instead of using colours associated with models and in effect giving the same information twice it would be nice for the reader to have the proper attribution analysis highlighted instead

p.30, l.537 – management of what?

p.30, section 7.1 – are the assumptions in any particular order? If so, what is it, if not it might be worth saying that.

p.30, assumption 1) While you discuss later whether the grouping is justified or at least how it can be tested you do not comment on the GEV fit at all. Is that justified? Can we test that?

p.32, assumption 9) – this is the strongest assumption I think and one that could be tested by using e.g. the weather@home model you've mentioned before, might be worth discussing why that has not been used (I assume no suitable runs were available but given that it is part of all other studies of rapid attribution publicised under World Weather Attribution for good reason it is worth mentioning)

p.32, l.720 – Finally? It is not your final point here. . . .

p.33, ll.737-748 – this is a paragraph that could be easily written in a single sentence

p.33, l.751 – why did you not include the satellite data in the study? I'd assume it

[Printer-friendly version](#)

[Discussion paper](#)



shouldn't take longer to analyse than the stations.

p.34,l.761 – for the kind of analysis you do high resolution seems only one factor, ensemble size is the other

section 7.2 – this section is rather disappointing in quality & added value given that it contains no insights in vulnerability and the latter part reads like an acknowledgement. I would recommend rewriting and drastically shortening to say why your study adds value instead of listing at length all the things you haven't done. It would be sufficient to mention that impact modelling and vulnerability assessment would be a great complementation.

p.35,l.3303 – end – it does not become clear how a synoptic analysis would provide anything useful in terms of decision making unless it is coupled with an assessment of the likelihood of the synoptic systems to occur hence this paragraph becomes a bit incomprehensible, again it would be better to highlight the strengths of your study and maybe refer to the Otto et al. paper if you want to highlight that the topic is not uncontroversial

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-448, 2016.

[Printer-friendly version](#)

[Discussion paper](#)

